metrical development of the two sides of the body is of vital importance, and is strictly preserved by natural selection; but more or less defect of symmetry often occurs as a variation or monstrosity, and in cases where such asymmetry was useful these variations would be preserved and increased by selection and heredity. An altogether erroneous view is taken of the fact of effort being used in this case, as if it were something unusual. But in all cases selection produces changes which are useful and whose use is often indicated by effort. The giraffe uses effort in stretching its neck to obtain food during a drought; the antelope exerts itself to the utmost to escape from the leopard; but it is now recognized that it is not the individual change produced by this *effort* that is inherited, but the favourable constitution which renders extreme effort unnecessary, and causes its possessors to survive while those less favourably constituted, and who therefore have to use greater effort, succumb. In the case of the developing flat-fish also, the effort indicated the direction of the useful modification, and any variations tending either to the right kind of asymmetry or to a mobility of the eye, admitting its being twisted and retained in its new position, during the growth of the individual, would be certainly preserved.

I wish to take this opportunity of thanking Prof. Ray Lankester for his careful and appreciative review of my book. I am too well aware of my own deficiency in training as a naturalist not to admit all the shortcomings which he so tenderly refers to. It is quite refreshing to me to read at last a real criticism from a thoroughly competent writer, after the more or less ignorant praise which the book has hitherto received. I admit also that the term "laboratory naturalist," to which he demurs, was not well chosen. I meant it as the opposite, not so much to "field naturalist" as to "systematic naturalist"; and it still seems to me that the gentlemen he refers to as not being "laboratory naturalists" are still less "systematic naturalists," in the sense of having specially devoted themselves to the observation, description, and classification of more or less extensive groups of species of living organisms. ALFRED R. WALLACE.

A Mechanical Illustration of the Propagation of a Sound-Wave.

HAVING to prepare some lectures on sound, I wished, if possible, to illustrate, without any very complicated apparatus, the way in which a sound-wave is propagated.

The following method suggested itself to me. As I have not met with the method while examining a large number of works on sound and wave motion, I venture to send a description of it to NATURE, as it may perhaps be of use to some students of acoustics.

A row of pendulums of equal length, *a*, *b*, *c* . . . *l* (Fig. 1) are suspended from a rod AB; in order to start the pendulums,

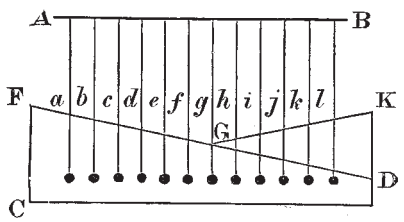

FIG. 1.

the bobs are held against an angular-shaped board, FCD, the rod being held in a plane slightly behind the plane of the board; if now the rod and pendulums be raised together vertically, *l* will first swing, then *k*, and so on, till all are free: when the pendulums are raised with a uniform velocity, then each pendulum starts at an equal period of time after the one which is next to it; the result is that a wave-motion is seen to run along the line of bobs as they vibrate to and fro. Such an arrangement has been used to illustrate wave-motion, as each bob moves with harmonic motion. But such an arrangement does not illustrate directly those compressions and rarefactions whereby sound is propagated. A slight movement, however, of the rod at once makes it do so. If, while the pendulums are vibrating, the rod from which they are suspended be turned in the horizontal plane through a right angle, the direction of the swing of each pendulum is not changed, and all the pendulums swing in the same plane. This will become clear from (Fig. 2), where the pendulum bobs viewed along OX appear to trace out wave-motion; the relative position of the bobs after the rod which supports them is turned through a right angle is shown along OY; the motion then illustrates mechanically those movements of air-particles which, when in compression and rarefaction, propagate a sound-wave. If the rod be turned back through a right angle, the wave-motion is again restored. The illustration must be taken with the obvious defect, viz. that the bobs move in arcs, and not in straight lines.

Care should be taken that the amplitude of vibration be not greater than the distance between the points of suspension minus

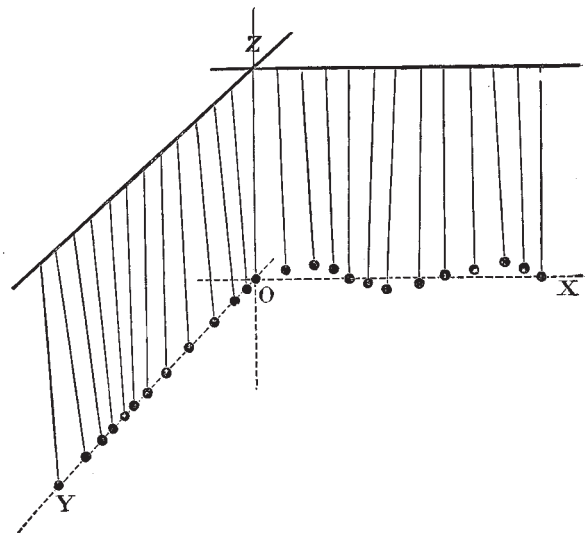

FIG. 2.

the diameter of a bob, otherwise the bobs will hit each other when vibrating in the plane YZ.

Twelve pendulums made of lead bullets 1·5 centimetre in diameter, suspended from threads 30 centimetres long, with a distance between each of 5 centimetres, were found to answer well by the author.

If the board used for starting the pendulums be made of the angular shape, FGK, then the movement of the bobs in their second position illustrates the propagation of sound on each side of its origin. FREDERICK J. SMITH.

Trinity College, Oxford, October 1.

On some Effects of Lightning.

THE twisting of one of the two trees near St. Albans, which were struck in such a remarkable manner by lightning, may well have been caused by the fall of the top of the tree, as Mr. Griffith suggests, and not directly by the lightning.

I have been unsuccessful in ascertaining whether the core of the tree is situated nearer that side where the explosion seems to have been most violent; but a more detailed examination only enforces the conclusion which Mr. Griffith and I arrived at, that the explosion must have occurred inside the stem, if not actually at the core of the tree.

The effects in this case can meet with no explanation from the supposition that the lightning passed between the bark and the tree, generating thereby sufficient steam to blow off the bark and shatter the stem—an explanation which Mr. Maclear suggests in his letter of September 25. I doubt if any source of heat would ever convert water so quickly into steam as to endow it with the power which dynamite has of shattering a hard object lying in contact with it, while the gases formed are restrained by the comparatively feeble resistance of the bark and outer air; nor can we suppose that sufficient heat could pass into the stem to generate steam there adequate for such an explosion, even if the uncharred condition of the wood did not prove incontestably that the temperature had not been raised very high. It seems more probable to me that such explosions must be caused by the lightning electrolysing the liquids in the stem, and

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thereby causing the sudden production of large volumes of gases at the ordinary temperature.

In answer to Mr. Griffith's query, I may state that the two trees are 34 yards apart; that there is no other tree in a direct line between them, though there are two about 4 yards from this line, and about midway between them; that the trees are certainly not isolated in any way, since there are fifteen trees within less than 34 yards of one of them, and about the same number within the same distance of the other.

48 Bryanston Square. SPENCER PICKERING.

Yew Trees in Berks.

A COMMUNICATION from Mr. Walter Money respecting two yew trees which were planted in the churchyard of Basildon, Berks, by Charles, Lord Fane, in 1726, has appeared lately in some of the papers (*North Wilts Herald*, October 4, 1889, and *Standard*), in which he refers to the dimensions recorded in the parish register, taken in 1780 and 1796, and again by my father in 1834. He adds his own observations on the growth taken this year. As I happen to have the original notes made by the late Prof. J. S. Henslow, dated "1834, August," it may be not uninteresting to record them. He writes as follows:—

"Measurements of yew trees at Basildon Churchyard, planted in 1726; taken near the ground:

		Ft.	In.	
Tree to south,	1780	6	3	According to Register.
	1796	8	6	
	1834	8	9	[J. S. H.]
	,, (at 4 feet)	6	10½	
	[1889	9	10	Mr. Money]

Tree to north [1780 and 1796 not recorded in register]:

		Ft.	In.	
Roots lately injured by digging graves.	1834	9	2½	[J. S. H.]
	,, (at 4 feet)	6	9¾	
	[1889	9	6	Mr. Money]

"From the three observations of 1780, 1796, and 1834, it would appear that the period of rapid growth stopped about 1796; but it seems probable that the measurement here is somewhat too great compared with that of 1780, as well as with that of 1834; for

Growth to	Ft.	In.		Lines.		Lines.		Years.	Lines per ann.
1780 ...	6	3	=	900	gives	300	diam. of gr. in	54,	*i.e.* 5½
1796 ...	2	3	=	324	,,	108	,, ,,	16,	,, 6¾

"Allowing this measurement to be wrong by 6 inches, it will reduce it to a greater probability also with that of 1834; and we shall have—

Growth to	Ft.	In.		Lines.		Lines.		Years.	Lines per ann.
1796 ...	1	9	=	252	gives	84	diam. of gr. in	16,	*i.e.* 5¼
1834 ...		9	=	108	,,	36	,, ,,	38,	,, 1
[1889 ...	1	1	=	156	,,	52	,, ,,	55,	,, 1]

"N.B.—The increase between 1780 and 1796 is too great, supposing the same parts to have been measured; and between 1796 and 1834 it is too little; therefore 1796 either took in too much of the circumference of the roots, or perhaps 1780 a little above them. Possibly the soil had become somewhat raised since 1796.

[Since 1834 the growth for the last fifty-five years will be seen to be exactly the same per annum, or 1 line.]

[With regard to the rate of increase at a height of 4 feet from the ground, he adds the following additional note.]

"Now the rate of increase of 4 feet from the ground is slower than that near the root, upon the whole, in the proportion of one-fourth, nearly. Taking, therefore, this fact with the indications given above, we may average the growth of the stem at 4 feet in the following way:—

"Diameter at 4 feet = $\frac{990}{3}$ lines = 330 (in 1834). Dividing this by the age, or 108 years, it gives 3 lines per annum nearly.

"Also 1780 to 1796 gives 84 lines for 16 years, *i.e.* 5 lines per annum.

"As it seems not to have grown much in the last twenty-eight years (*i.e.* up to 1834), if we allow 1 line for this period, and distribute for the eighty years of rapid growth, we get the following result; thus:—

First 20 years at	3½ lines	=	70	Young growth.
,, 40 ,,	4 ,,	=	160	
,, 20 ,,	3 ,,	=	60	
,, 28 ,,	1½ ,,	=	42	Settled period."

GEORGE HENSLOW.

Maxwell's "Electricity and Magnetism."

THERE is apparently a trifling slip in § 360 of Maxwell's "Electricity and Magnetism." The ratio of the resistance of pure iron at 100° C. to the resistance at 0° C. is there stated to be 1·645. This ratio is evidently calculated from the results given in Matthiesen's paper on the influence of temperature on the electric conducting power of thallium and iron (Proc. Roy. Soc., 1862–63). The true ratio for pure iron annealed in hydrogen is 1·6255. The other ratios mentioned in the paragraph are correctly deduced.

HERBERT TOMLINSON.

King's College, Strand, October 12.

AN EXAMINATION OF SOME POINTS IN PROF. WEISMANN'S THEORY OF HEREDITY.[1]

PROF. WEISMANN'S views on heredity and allied phenomena have met with such general acceptance that I feel it to be presumptuous on my part to attempt any criticism of them. I cannot but think, however, that a statement of the difficulties which they present to me, and of the inconsistencies which appear to exist in the argument, may be of value, not indeed as a refutation, but as drawing attention to those points which seem to require further elucidation.

It will be necessary for me to state Prof. Weismann's argument, and I shall endeavour, in so doing, to represent it as fully and as fairly as my apprehension of it will admit, and as far as possible in his own words. But this is a matter of no small difficulty, inasmuch as the argument has to be traced through a number of separate essays, even though these essays have been collected into one volume and translated into English. All the references which I make relate to the English edition.[2]

The fundamental fact upon which the whole argument is based, and which Prof. Weismann appears to have fully established, is that the body of unicellular organisms (monoplastides), as also that of undifferentiated multicellular organisms (homoplastides), is immortal, at any rate potentially. This position is clearly stated in the following passage (p. 25):—

"The process of fission in the Amœba has been recently much discussed, and I am well aware that the life of the individual is generally believed to come to an end with the division which gives rise to two new individuals, as if death and reproduction were the same thing. But the process cannot truly be called death. Where is the dead body? What is it that dies? Nothing dies; the body of the animal only divides into two similar parts, possessing the same constitution."

Death is, on the contrary, a characteristic feature of differentiated multicellular organisms (heteroplastides); but even in these forms there is still an immortal part, for the reproductive cells which develop into new individuals are evidently as potentially immortal as the Amœba. In these higher organisms, therefore, the mortal cells are to be distinguished from the immortal. This distinction is drawn by Prof. Weismann as follows (p. 122):—

"It is necessary to distinguish between the mortal and the immortal part of the individual—the body in its narrow sense (*soma*) and the germ-cells. Death only affects the former; the

[1] This paper is an expansion of some remarks contributed to the discussion on "The Transmission of Acquired Characters," which took place in Section D during the recent meeting of the British Association at Newcastle.

[2] Weismann, "On Heredity" (Oxford: Clarendon Press, 1889).

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